OBITUARY NOTICES

OF

FELLOWS DECEASED.

CONTENTS.

Keith Lucas.

KEITH LUCAS, 1879-1916.

Part I.

Keith Lucas was born March 8, 1879, and was killed on October 5, 1916, when he was 37 years old. His death was caused by a collision with another aeroplane in mid-air, when flying over Salisbury Plain, and was instantaneous.

He was the son of Francis Robert Lucas and grandson of Ralph Willett Lucas, a Lieutenant in the Royal Artillery who fought in the Battle of Waterloo. His father, Francis Lucas, went as engineering pupil to the Telegraph Construction and Maintenance Company, East Greenwich, when he was 15 years of age and was one of the young engineers chosen to go out in the S.S. "Great Eastern" when the first Atlantic cable was successfully laid. He afterwards went in her on every voyage. After this he was made manager of the works at Greenwich, but continued to go to sea and lay cables until he had laid the Pacific or "All Red Cable" round the world. He then gave up the work at sea and became Managing Director of the Company. During his life at the works he was continually inventing and improving the machinery for cable manufacture and cable laying. Many of his inventions have been adopted by the Admiralty as well as by cable companies.

Keith Lucas' great-grandfather on his mother's side, Edward Riddle, and his grandfather, John Riddle, were both good mathematicians, both were Fellows of the Royal Astronomical Society, and both had great reputations as teachers of navigation and nautical astronomy.* Lucas inherited from his ancestors his scientific interests and ability, his power of designing new apparatus, and his engineering capacity, as well as his great manual dexterity.

From the Rev. T. Oldham's preparatory school at Blackheath, he gained a Classical Scholarship at Rugby in 1893. He was in the Volunteers at Rugby, and in the Shooting Eight for four years; and was captain of it in 1897-8. He was head of the School House before he left to come up to Trinity College, Cambridge, with a Minor Scholarship in Classics.

His interest in Engineering and Science was encouraged at home, where he learnt the use of many tools. His work was from the first neat and practical. His mastery of the design of new instruments, coupled with his capacity of making what he wanted, gave him great power in the art of experiment in such dissimilar sciences as Physiology and Aeronautics.

At Cambridge he devoted himself to Science, and took a First Class in the Natural Sciences Tripos, Part I, 1901. After the Tripos, he felt the strain of doing so much work in a new direction, and at this time his old school friend,

* 'Dictionary of National Biography' and 'Monthly Notices of the Royal Astronomical Society,' February, 1855; 'Monthly Notices of the Royal Astronomical Society,' February, 1863.

Charles Powell, was killed in the Boer War. Lucas felt his death acutely. He gave up working for the second part of the Natural Sciences Tripos and went to New Zealand for the sake of rest and change. There, he measured the depth of some lakes reported as bottomless, and when he returned to England he published the paper, "A Bathymetrical Survey of the Lakes of New Zealand," in the 'Geographical Journal,' May and June, 1904. His father's experience in deep-sea sounding and cable laying was naturally helpful to him. This was the beginning of his original work. The advances he made in Physiology are described in a separate Memoir, and were not interrupted till the war induced him to devote his abilities to Aeronautics.

He was elected a Fellow of Trinity in 1904 and in the same year gained the Walsingham Medal and the Gedge Prize. He was appointed Lecturer in the College in Natural Sciences and took the Degree of D.Sc. in 1911. He was elected a Fellow of the Royal Society in 1913, having already delivered the Croonian Lecture in 1912. He also gave the Page May Memorial Lectures in connection with the Institute of Physiology, University College, London, in 1914.

He felt that all should take their full share in the work connected with the administration of the University and College business. He was on the Council of his College for some years, spoke little, but always to the point, and took his full share in decisions, and his opinion was highly valued. When the new Physiological Laboratory at Cambridge was being planned, and later, before it was occupied, he did much to make the arrangements satisfactory and efficient.

Keith Lucas was keenly interested in and enjoyed his work as a teacher, both in his College classes and in his University lectures and demonstrations. He felt the importance of this work, and, with his knowledge of the difficulties of the subject and his power of clear explanation, it was natural that he should be a most successful teacher. The work he had himself done in Physiology, and his hope for far greater advances, both by his own work and the work of others, inspired in his pupils enthusiasm for further investigations.

In 1909 Keith Lucas married Alys, daughter of the Rev. C. E. Hubbard. He leaves three sons. He became a Director of the Cambridge Scientific Instrument Company in 1906, and only resigned in 1914, when he joined the Royal Aircraft Factory. During this time he designed many instruments both for teaching and research.

Even when he was in New Zealand, and before he had begun his original scientific work at Cambridge, and had so much to do with the design of scientific instruments, he thought that the great flaw in instrument making was that there had to be much perfect and expensive workmanship to make up for faults in design. He agreed with the definition of a well designed instrument as one which worked smoothly and well, and gave accurate results when the rubbing surface became worn or the parts damaged, and even if badly made. In his designs, each moving piece was allowed only the

requisite freedom to move in the proper manner, and was guided only at the correct number of points. The importance of these considerations was long ago pointed out by Clerk Maxwell and Lord Kelvin, and generally leads to a good design as defined above.

In his work with the capillary electrometer, he had to analyse a great number of photographic curves; to do this he designed an instrument which saved a great deal of time, and gave results with remarkable accuracy. In connection with this work, he designed a rapid and trustworthy method of drawing fine capillary glass tubes. He also designed a photographic time-marker on the principle of the Einthoven string galvanometer, in which the self-induction and inertia were much reduced, and the time-lag was extremely small. Among other instruments, he designed apparatus for breaking two electric contacts at short intervals apart, and many useful instruments for teaching.

Like so many of the best experimenters, he made with his own hands much of the apparatus he wanted for research, and his skill allowed him to use the simplest means to get good results. Some of the more elaborate instruments, however, were made by the Cambridge Scientific Instrument Company, and he often discussed the designs with me, as Chairman of the Company and as his personal friend. This was always a pleasure; he knew what the instrument should do and how the parts should be made, and his quickness in realising the difficulties, and in seeing improvements in a mechanical design, was most striking. Sometimes I felt proud that I was able to improve the work of such a master of the art of design. If this happened to be the case, his quickness and fair-mindedness made him realise the fact at once. If it were not so, he very soon proved that he was right. All he wanted was to get the best out of whatever work he was doing; it was the good quality of the work he cared for, not any personal credit in connection with it. This tendency to think of his work and not of himself was appreciated at the Royal Aircraft Factory, and is described in the 'Court Journal' of October 20, 1916, where the writer adds: "I wish there were more like him in that way."

When the war broke out he at once became a volunteer, and did useful work in patrolling the road to Newmarket on a motor bicycle. He then passed the medical examination for the Honourable Artillery Company, and was about to enlist as a private in the infantry battalion. Just then I happened to meet him, and realised at once that his value to the country would be far greater if he worked at the Royal Aircraft Factory, and a telephone conversation with Colonel O'Gorman, Superintendent of the Royal Aircraft Factory, the same day, removed all doubt, and he began work at once.

His scientific training and attainments, his knowledge of the manufacture of scientific instruments, and his remarkable powers of design and research, enabled him to do most valuable work there. Colonel O'Gorman, C.B., Keith Lucas' commanding officer, and Superintendent of the Royal Aircraft Factory during the two years he worked there, writes:—

"Shortly after the outbreak of war, amongst those who flocked to the colours was Keith Lucas. He was a straight, well-knit man, slender, but active, with a body exceptionally finely controlled by an energetic mind. He was young for the manifold scientific honours which distinguished him, but he was so far from over-appreciating himself that at one time he was about to enlist as a private. But, instead of this, he was able to use his rare scientific abilities in improving military aeronautics.

"By a lucky chance, I had the opportunity of giving him work at the Royal Aircraft Factory after he had passed the medical examination for the Honourable Artillery Company.

"I had previously met him when staying in Cambridge, and had heard him discuss questions of mechanical design. This was enough to leave no doubt of his utility, and I seized the suggestion of his joining the Royal Aircraft Factory.

"For Lucas this meant leaving his home at Cambridge, and giving up his original scientific work in Physiology, which was the dominant and all-absorbing interest of his life.

"He arrived at the Royal Aircraft Factory on September 4, 1914, and after living at Fleet for some months, took up his residence in the little wooden hut, 12 feet by 10 feet, which was the only possible means of being housed in the crowded neighbourhood, where workpeople were sleeping as many as 11 in a six-roomed cottage, or using in pairs, for alternate day and night work, the same beds. To be near his work was essential, as in summer he was often flying at dawn.

"He was entrusted with one problem after another in rapid succession, while, simultaneously with this, he held himself open to be consulted, and was constantly consulted, on numerous problems.

"The Experimental Research Department was evidently the place where his abilities would have scope, and there he took up his work. This was the department which had so lately been presided over by Edward Busk, a distinguished graduate of King's College, Cambridge, with whom were associated a number of other Cambridge men, with whom Lucas enjoyed working.

"In breaking new ground of the kind to be dealt with in this department many of the steps meant making measurements of quantities which had never before been measured, and in this he must have found a link with the analogous difficulties in his own study of Physiology. Methods had to be evolved, and instruments to be designed and made. Here Lucas excelled, and in the instrument shop, under Mr. F. Short, he was welcomed and honoured by staff and mechanics alike.

"It chanced that the problem of how to make an accurate sight for dropping bombs from aeroplanes was under consideration shortly after Lucas came. He worked at this, and the seed has been sown which will greatly improve the aeroplane as an offensive weapon.

"On the way to his solution, by means of the gyroscope, he evolved his 'space damped pendulum,' which is in large measure free from the effect of

the movements and oscillations which the aeroplane imposes on everything within it. This was a simple device, which avoided the complexities attendant upon the use of gyrostats, and one of its practical outcomes was a new instrument, an aeroplane level, of considerable use in a number of further experiments on aeroplane flight.

"The trend of all this work indicated how necessary it was to obtain an autographic record of the movements, of roll, pitch, and yaw of an aeroplane, both when the pilot abandoned all control, and also when the pilot exercised his utmost vigilance in correcting all deviations. This Lucas was asked to do; he completed a method outlined for this purpose by Mr. Busk, and simplified it. He worked hard, rising day after day at four in the morning for flights when the air was at its stillest and sun low, and, in conjunction with Captain Mayo and Major Goodden, eventually produced a beautiful series of curves of motion which were sent in to the Advisory Committee on Aeronautics, and were received with marked approval.

"With these data, he now knew what classes of erratic motions he had to deal with, when either the flyer or the wind gusts interposed to upset or alter the aim taken with his bomb sight.

"By this time he had decided that the use of the more complex gyrostat could not well be evaded in favour of his simpler scheme of the 'space damped pendulum.' He adopted a suggestion of Major Hopkinson, F.R.S., who had also been for some months a member of the Royal Aircraft Factory staff, and made an improved bomb sight.

"A new and somewhat startling difficulty had been found in connection with aeroplane compasses. Fliers, lost in the fog or cloud and persisting in what they thought was a careful compass course, would find themselves facing in a direction opposite to that in which they believed they were—they would come out of a cloud where they went in without having deviated from a compass course which should have taken them straight through it. This was given to Lucas to solve. He first found the causes of the erroneous indications, and then made a compass in which they were greatly reduced; and his 'space damped pendulum' inspired one part of the remedy. A portion is due to Mr. H. Darwin, F.R.S.

"Lucas was a master of clear and lucid exposition, so that, though he was not always easy to draw into any long dissertation, if he once decided to state a case, there was no loophole for misunderstanding him.

"On the formation of a Territorial Unit of the Royal Flying Corps recruited from the employees of the Royal Aircraft Factory, Lucas was one of the first to be approached by myself, as officer in command, with a view to his taking command of a Park—roughly, 400 men. He willingly consented to take the additional work and responsibility. He was gazetted Captain on December 1, 1916, and appointed to the command of No. 3 Park. He threw himself whole-heartedly into his military duties, and this was soon reflected in the discipline of his command.

"He was essentially a popular officer, and this in a military sense. His

men had implicit confidence in his ability to lead them, and no greater tribute is needed.

"I invited him to be the first mess president of the officers' mess, and he retained this office till his death. He had a good influence over the younger officers, and his loss will be greatly felt.

"He was a regular attendant at the Commanding Officer's lectures, and in his own lectures on technical subjects no officer held the attention of his audience on seemingly dry subjects so perfectly as Lucas. His clear style and unusual form of wit made his subject interesting to the latest promoted non-commissioned officer.

"His request to be allowed to learn to fly was granted, and he went to the Central Flying School, where he acquired the art remarkably quickly. He never had a mishap until the fatal collision in the air, when the air-screw of the other aeroplane struck him. He was undoubtedly killed instantly."

Colonel O'Gorman has pointed out the value of Lucas' work on aeroplane compasses. An error, which we will call "the turning error," had often been noticed on aeroplanes. When an aeroplane turned to the right or the left the compass did not indicate the magnetic north correctly. He not only found the cause of this error but designed and made a compass which reduced the error to a great extent. It was, however, a disappointment to him that he was not able to eliminate the error completely. The first difficulty to be overcome was to find out why the error was capricious; sometimes it showed itself and sometimes it did not, and it became clear that although it depended on the rate of the turn, it also depended on something else. After much flying and observation of a compass in the air, he found that this error was very great if a deviation was made when the direction of flight was towards the north. In this case the compass needle was so far carried round with the aeroplane, when on a turn, that the flier might think he was flying in a straight line, although he was turning somewhat rapidly. In a cloud, or at night when there were no visible objects to act as guides, this was a great danger. If the aeroplane was flying in a southerly direction the compass needle turned in the opposite direction, and the flier would get an exaggerated estimate of his rate of turning. As the flier's object is to fly straight through a cloud this would not matter.

The abnormal behaviour of the standard compass in the air was utterly unexpected, and the value of the discovery was great. In order to remedy this defect, which was found in all existing compasses, much experimental work had to be done in the air; he formed theories of the cause of the error, tested them in the air, and after eliminating those which proved wrong, at last found the true cause.

The magnetic forces act on the poles of the magnet in the direction of the dip, and tend to rotate it in a vertical plane. When the aeroplane is flying straight this tendency is balanced by displacing the centre of gravity towards the south pole, in order to keep the magnet horizontal. But when

the aeroplane is turning the apparent direction of gravity is no longer the true vertical; the magnetic forces, however, still act in a vertical plane, as before, and this change of conditions produces the turning error.

The compass that Lucas made was a great improvement on the existing patterns. Its special features are the combination of the antivibration mounting; the spherical bowl to contain the liquid; a magnetic system small in relation to the size of the bowl, with a long period of vibration; graduations on a short cylinder instead of a disc; and the inverted pivot.

Unknown to Lucas, most or all of these features had been tried before for marine compasses; but they are not called for at sea, owing to the relatively slow speed of ships, and had been long forgotten. By his work he brought them into use for aeroplane compasses, and they are an important life-saving factor.

One of the reasons why vibration causes errors in compasses was pointed out by Mr. A. Mallock to the Advisory Committee for Aeronautics, and it fell to me to be of some little assistance to Lucas with regard to this error. Theoretical considerations showed that this vibration error would be reduced by inverting the usual arrangement in compasses in which an agate cup is carried by the card and rests on a needle-point fixed to the compass bowl. If the needle-point is fixed to the magnetic system and the agate cup is supported by the compass bowl the vibration errors were reduced. Lucas looked into the theory of the vibration error, confirmed the experiments I had made, and adopted the inverted point support, and it is a great satisfaction to me to have been of some use in this improvement.

Many of Keith Lucas' friends heard, with regret, that he was learning to fly. In addition to their personal affection they felt the possible loss to science owing to the risk he was running; they also thought that the advances he was making in aeronautics were so important that no chance of interruption by an accident should be taken. But it was questionable whether the risk was increased. Before he learnt to fly he had been in very many flights on an aeroplane as a passenger when he was experimenting with various instruments, and for this work it was essential that he should be a passenger. When flying as a passenger an accident might happen through want of skill of the pilot; when he was a pilot his own want of skill might cause an accident, but those that knew him felt sure that when he had once learnt to fly, he would have far more than the average skill in manipulation of an aeroplane, requiring as it does a clear cool judgment and rapid co-ordination of muscles and brain. He thought he could do his work better by becoming a pilot, and improve the technical part of the branch of the service to which be belonged, and he was right.

Flying as a passenger gave him great pleasure, even on the first occasion. But the pleasure in his first flight alone—his instructor left behind—was far greater still, and he met his death swiftly and suddenly in the open air doing the work he loved. He was buried in the Military Cemetery at Aldershot.

HORACE DARWIN.

Part II.

Although the physiology of muscle and nerve, and the nature of the excitatory process which passes along such tissues in the form of a wave from a stimulated point, had been subjected to investigation by a large number of workers, it is remarkable how little advance had been made since the time of Helmholtz and Du Bois-Reymond. An occasional fact of importance was discovered from time to time, but it was not until Keith Lucas commenced his systematic study of the process in 1903 that any rapid progress took place. In his first paper, which was devoted to the question of the effect of tension on the duration of muscular contraction, we find from the outset how great a part the design of appropriate and accurate instrumental aid was to play in the elucidation of the various problems attacked. It was by the elimination of the inertia of recording levers by the use of a photographic method that it was shown that increase of tension, within limits, results in a lengthening of the period of contraction. This fact, at a later date, was destined to play an important part in the theory of muscular contraction.

Gotch had already obtained results which indicated that the different degrees of contraction which a muscle is able to exert were due to the varying number of individual fibres at work, and not to the capacity of each fibre to contract otherwise than to the maximal extent within its power at the time. Keith Lucas brought forward convincing evidence that Gotch's contention was correct, in that the number of degrees of contractile stress possible for a muscle to manifest is not greater than the number of nerve fibres supplied to it. In a subsequent paper, description is given of experiments on a muscle whose nerve contains only eight or nine fibres. In this case, the nerve itself was stimulated. Thus the contraction of voluntary muscle was brought into line with that of the heart and Bowditch's "all or nothing" law shown to apply. Still later, Adrian, a pupil of Keith Lucas, was able to extend the law to the nerve fibre itself, by the use of an ingenious method to be referred to below.

The next step in the theory of contraction was to show that the wave does not change in magnitude during its passage, so far as normal muscle is concerned, although it may suffer diminution in fatigued muscle.

An important series of papers claims our attention at this stage, a series which may be said to have their starting point in the observations of Waller that the amount of energy required to stimulate a nerve varies with the rate at which this energy is applied. Different nerves have a different 'characteristic," due to the natural rate of movement possessed by some constituent, which rate controls the effective taking up of the incident energy. Keith Lucas' first experiments were made, as were those of Waller, by the use of condensers of adjustable capacity, charged to different potentials. Subsequently, it was found better to use as index the potential required with currents of varying durations, a factor related to the former in a definite way,

since the energy is expressed by v^2t, where v is the potential and t the duration of a current. Similar results were obtained with a simple apparatus designed to vary the rate of increase of an applied current. The methods described were utilised in the analysis of complex excitable systems, such as the sartorius muscle with its nerve. It was found that the muscle has two distinct optimal rates of incidence of energy, one of a very much greater magnitude than the other. This statement still held after sufficient curare had been given to abolish the effect of stimulation of the nerve trunk, so that there must be some additional excitable substance situated between the nerve and the muscle. By testing a part of the muscle free from nerve endings, it was found that the low rate belonged to the muscle fibre itself. The nerve trunk was found to have an optimal rate rather higher than that of muscle, while the intermediate substance had an extremely high rate. The form of the curve expressing the time-course of the relationship between duration of current and potential required to excite, as the muscle changes after excision, was shown to be altered by the presence or absence of calcium. This fact was brought into relationship with Nernst's theory of excitation, to which more attention was given later.

Another property of excitable tissues which is connected with the time-factor in question is the summation of two stimuli, each just below effective strength. If the excitatory process set up by the first stimulus has not disappeared when the second arrives, there is summation. So that the fact depends on the rate of subsidence, or, in the terms of Nernst's theory, on the rate at which the concentration of ions brought about by the exciting current is again dissipated by diffusion. A further outcome of this point of view was the analysis of various excitable tissues in the light of A. V. Hill's modification of Nernst's theory, in which account is taken of the distance between the membranes at which the ions concerned are supposed to be concentrated and of the diffusion constants of these ions. The time-factor turned out to be in reality conditioned by these two components of the expression deduced by Hill. An interesting point, as yet not explained, is that the rates of movement of the ions in different excitable substances differ more than those of the ordinary inorganic ions known to be present.

Since the rate of the excitatory process should be increased by rise of temperature, it was natural to bring the different effects of temperature on the apparent excitability of muscle and nerve to constant and induced currents into connection with their optimal rates of incidence of energy. The explanation was found to consist in two opposite effects of fall of temperature. A fall of temperature means, on the one hand, a greater ease of the production of the necessary concentration of ions, owing to the decrease of opposing diffusion, while, on the other hand, the actual initiation of the propagated disturbance is more difficult. The resultant effect varies according to the duration of the current required to excite a particular tissue.

The temperature coefficient of the rate of conduction in nerve was

measured by Keith Lucas, using an extremely accurate method. It was found to be 1·79 for the 10 degrees between 8° and 18° C.

For further progress it was necessary to make use of the electrical change in excitable tissues as indicating the excitatory process. For this purpose an improved form of capillary electrometer was invented, together with apparatus for measuring the curves for the purpose of analysis. It was first shown that the temperature coefficient of the rate of conduction in excitable tissue is the same as that of the time of development of the electrical disturbance. Hence there is no difficulty in taking this latter as the basis of propagation of the excitatory state, although no proof of their identity is given thereby.

The next series of papers are devoted to the refractory period which follows an effective stimulus. It was shown that the time which elapses before an electrical change shows itself, when it is due to a second stimulus following a previous one, is constant, although the time after the end of the refractory period at which the second stimulus is given may vary considerably. This fact suggested the name "irresponsive period" for the interval between an electric response and the earliest possible succeeding one. This delay occurs also in ventricular muscle, and is due entirely to a modification of the tissue by the preceding propagated disturbance, and not to any direct effect of the current used for stimulation. In conjunction with his pupil, Bramwell, Keith Lucas next showed that the decrease of excitability after an effective stimulus is due to the passage of the propagated disturbance itself. This refractory period is, in fact, independent of whether the two stimuli fall on the same point or on different points.

A peculiar phenomenon, known as Wedensky's inhibition, also found its explanation in Keith Lucas' work. It was shown to be due to a resistance to conduction in the nerve greater than normal, and the possibility of similar conditions in the nerve centres was discussed as a probable basis of some inhibitory phenomena.

A further step was taken, in conjunction with Adrian, towards the analysis of the process of stimulation, in that this was shown to consist of two stages, a purely local effect and a propagated effect. The former may be present, although insufficient to set in motion a propagated disturbance, and the fact that it does not immediately disappear renders possible summation of stimuli, each in itself an inadequate one. An important method was developed for the estimation of the magnitude of a propagated disturbance, as referred to above. This consisted in determining the distance travelled through a region in which it is progressively diminished in magnitude, that is, a region of decrement, such as is produced by anæsthetics. This method was afterwards applied by Keith Lucas himself to the decision of the important and disputed question as to the possibility of distinguishing between conductibility and excitability in nerve, a distinction which, it had been stated, could be made out by the use of alcohol as a narcotic. The two factors were shown in reality to disappear together, and this disappearance

to be due to the same cause, namely, increased difficulty in setting up a propagated disturbance. It was also found that, when such a nerve impulse is set up by a strong stimulus in the "relative" refractory period, it is smaller in magnitude than the normal one, produced by a stimulus outside the refractory period.

The Croonian Lecture was given by Keith Lucas in 1912, and was devoted in part to a discussion of the work referred to in the preceding pages, and to the investigations of other workers on questions related to it. The Lecture concluded with a more detailed description of the modified form of Nernst's theory of excitation, and with criticism and suggestions concerning it.

In a valuable paper on "Summation in the Claw of the Crayfish," published in the 'Journal of Physiology' after the author's death, some of the facts previously discovered are made use of to elucidate the complex problems of this interesting neuro-muscular mechanism. By application of the law governing the relation of current strength to the time of closure required to stimulate, it was found that there are two sets of nerve fibres in the nerve to the claw. One of these is responsible for a slow prolonged form of contraction, the other for a brief quick twitch. It was also found that the type of summation described by Richet was due to the fact that the first stimulus, although sending an impulse along the fibre, fails to cause contraction because it has been reduced in intensity by having to pass through some area of decrement on its course, probably at the synapse of the nerve fibre with the muscle fibre. A second stimulus, however, applied to the nerve during the period of increased excitability following the previous stimulus, produces a disturbance of sufficient magnitude to pass the obstruction, and contraction of the muscle results. This period of super-normal excitability, succeeding the relatively refractory phase, is more strongly developed in the crayfish than in the frog.

In a footnote to this paper, reference is made to some experiments on the phenomena of inhibition and of tonus shown by the muscles of the crayfish claw. It is much to be hoped that notes of these experiments may be found sufficiently detailed to enable them to be published.

Early in 1914 Keith Lucas gave the Page May Lectures at University College, London, choosing as his subject the phenomena of conduction in nerve. At the outbreak of war, the greater part of these Lectures had been written out for publication as one of the monographs of Prof. Starling's series. After the author's death, the manuscript was revised and completed by Captain Adrian, and the Lectures, which give an excellent summary of the knowledge gained in this field, for the most part by the work of the author and his pupils, were published towards the end of 1917.

It will be seen how great a loss physiological science has sustained in the death of so ingenious and talented a worker at so early a stage of his work. Many further and fundamental advances would undoubtedly have been made if only these researches could have been continued. W. M. Bayliss.

HUGO MÜLLER, 1833–1915.

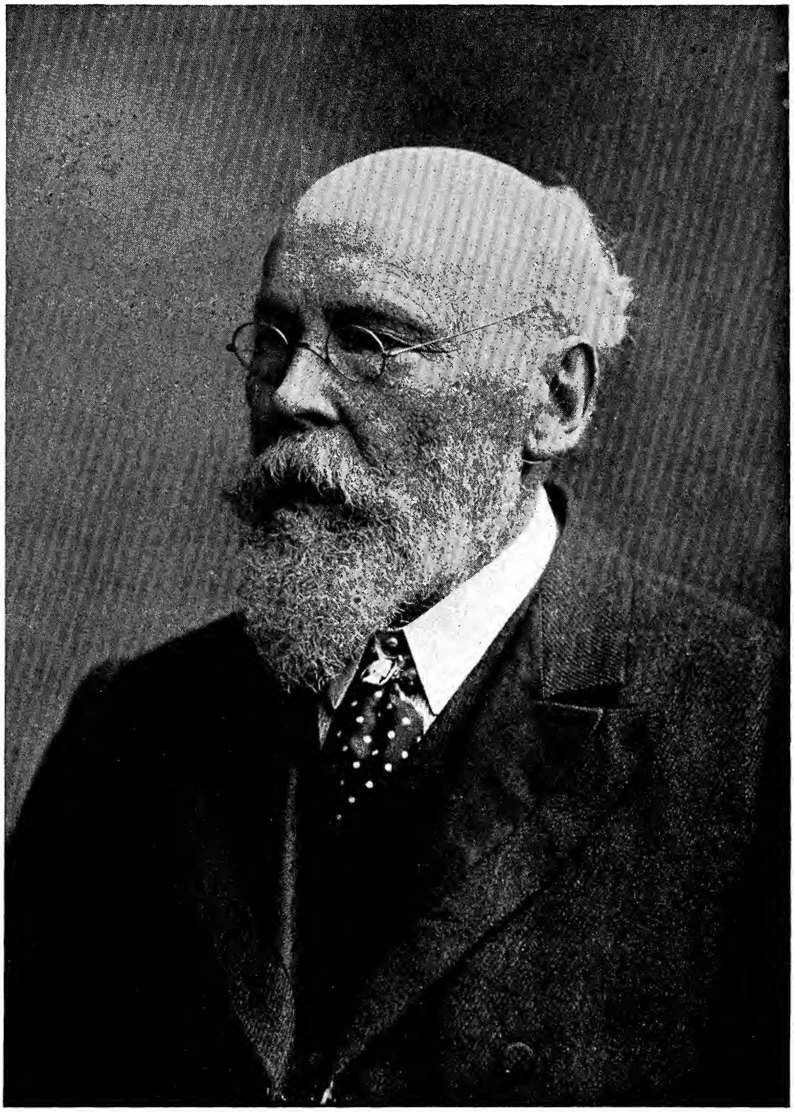
Of the distinguished Germans I have known well, Hugo Müller was one of the most attractive and interesting—on account of his many gifts and his kindly, sympathetic nature, guarded though this was by a peculiarly cautious reticence. In the true sense of the term, he was a born naturalist—most happy when musing over minerals or among plants, especially when among the latter, on Swiss or Tyrolean mountain side or in his garden; very happy also, particularly in the last dozen years of his life, when in the chemical laboratory, seeking to contribute to the solution of the problems which Nature ever held up before his eyes.

A man self-contained and of a reflective disposition, eminently sparing and frugal in his habits after the manner of his race, simple and unworldly and above ordinary ambition, a hater of display although in no way averse from the advantages and comforts accompanying wealth, always affable and fond of congenial, sympathetic society: he looked on at our world of strife, noting its ways, often with disapproval, yet lived a life apart, mainly introspective and rarely assertive though it was in him to be obstinate if occasion arose, finding joy chiefly in the conscientious prosecution of his work and in following the growth of knowledge, with utmost interest and appreciation, even unto his last days, although after his retirement from business he ceased to be the omnivorous reader he was in early days.

When I first learnt to know him, at the beginning of the 'seventies, on my return from a German university, he at once attracted me and I was greatly encouraged by his kindness; there was no Englishman to compare with him for breadth and accuracy of reading; no one so sympathetic or with whom it was possible to talk shop with so much satisfaction. Perhaps this was because, in those far-off days, among the frequenters of the Chemical Society, he was the one chemist in touch with practice and in touch also with the outdoor world, yet mindful, to an extraordinary degree, of the philosophy of our science.

He had artistic gifts of no mean order; these were of great service to him while associated with Messrs. De la Rue and Co., particularly in connexion with the development of lithographic colour printing for stamps. It may be mentioned, too, that he was a particularly deliberate and good writer and that the gift of calligraphy appears to have been inherent in his family—to judge from letters written by his relatives. He had also extraordinarily deft fingers and was the acme of neatness in all his ways; this was particularly noticeable in his laboratory work. He had the faculty of dealing with small quantities of material, though a great believer in having plenty at hand, whenever possible; except Peter Griess, I have not known anyone with so great a power of extracting crystalline products from masses.

Agnostic and truly scientific in habit of mind, cautious to the last degree



Hugo Müller

and possessed of a highly developed sense of accuracy he neither speculated nor dreamt but his powers of observation were very acute. Had he not been led, through the accident of environment, to devote himself to a technical career, it is probable that he would have followed the bent of his early inclinations; had he done so he would have taken high academic rank. His breadth of outlook and practical gifts would undoubtedly have enabled him, in his own country, to secure a position complementary to that his brilliant fellow-countryman and friend Kekulé achieved through his contributions to the theoretical side of our science.

A worthy pupil of his great master, Wöhler, Hugo Müller was a true representative of the old school of German scientific worker to which the world owes so much, a school which set a wonderful example of self-devotion, thoroughness, technical skill and patient industry.

At the outbreak of war, although he had been resident here since 1854 and married an English wife and had long settled down as a country gentleman, his sympathies reverted to his native country. But he set a praiseworthy example and withdrew himself entirely from society, retiring to Camberley and his garden. When I called on him, in January, 1915, shortly after my return from the East, to tell him about Australia and Java and Ceylon, and to discuss with him the wonders of their vegetation, I was his first visitor; afterwards he only saw one or two other old friends. We persuaded him with difficulty to return to the Davy-Faraday Laboratory but he only worked there during a brief period; fortunately, he had completed his work on Flavone and saw the paper through the press shortly before his death.

As proof of his nobility of character and high sense of duty, I may be allowed to quote from the letter of February 7th, 1915, in which he communicated to the Committee the reasons for his resignation from the treasurership of the Lawes Agricultural Trust, which he had held from the inception of the Trust in 1889:

"In the letter announcing my retirement, I gave my age and consequent waning efficiency as the cause of my decision in this matter and as I deemed this by itself quite a sufficient explanation, I did not expect that anything more than an expression of mutual regret would ensue. Letters, however, from some of the members and the signed request of the whole Committee make an earnest appeal to me not to give up my membership and in the embarrassment in which I thus find myself, I feel compelled to confess that my retirement is not wholly due to superannuation.

"The unfortunate circumstances which have arisen through the war have most seriously affected every German resident in this country whether naturalised or not and I am afraid my colleagues have not attached sufficient weight to the fact that, under the present conditions, it is not desirable for a person in my position to be a member or to take any part in the affairs of any public concern or enterprise.

"This is, of course, a matter arising out of the prevalent public feeling and probably does not affect to any extent the Lawes Committee; but still

it has to be reckoned with, for the latter has to cultivate the good will of the public as well as that of the Government. You will see from this that, in my opinion, any desire on the part of the Committee to retain me must 'stand back' and I can only repeat my deep regret that my separation has become necessary just now."

We were all most anxious to retain him with us but could not persuade him to change his mind. Always a peacemaker, he was torn with distress by the outbreak of hostilities and never ceased from lamenting the war as a hopeless downfall of our civilisation: he deplored the fact that he had lived to see it; his end was greatly hastened, if not determined, by dwelling on its awful consequences.

Heinrich Wilhelm Hugo Müller was born at Tirschenreuth, Bavaria, on July 29th, 1833. His father was the owner of a spinning mill. During his school days, at Nürnberg, he paid special attention to chemistry but the Fichtelgebirge near his home and, after his father's removal to Weiden in the Oberpfalz, the Bayerische Wald, also afforded him special opportunities for the field study of mineralogy and geology; it is clear that he made good use of these, as he then laid the foundation of his collection of minerals, to which he gave particular attention during the earlier years of his residence here, when he was one of a select set, mostly members of the B Club of which so interesting an account was given to the Chemical Society by Dr. A. Scott in his Presidential Address in 1916. Among these were Phipson Beale, David Forbes and Frederick Field, the brother-in-law of the late Sir Frederick Abel; Field had brought home many very beautiful rare silver and copper minerals from South America. Since his death the collection has been presented by his widow and daughter to the mineralogical department of the Oxford Museum. In so far as the crystallographic studies carried on in my laboratory have been of value, the opportunity of doing such work was secured largely through Müller's aid; as President of the Society in 1885–1886, he was *ex officio* a member of the Executive Committee of the City and Guilds of London Institute for the Advancement of Technical Education, at the time of the opening of the Central Technical College; it was mainly through the support he gave me that I was able to include crystallography as a necessary regular element in the full course of instruction in my department.

At the age of seventeen, in 1850, Müller entered the University of Leipzig. He studied chemistry under Erdmann, physics under Hankel and mineralogy and geology under Naumann. In the late 'sixties I heard the two latter and Erdmann's laboratory was still in existence—so I know exactly the conditions under which he was placed; they were the best of their time. Apparently, his first intention was to devote himself to geology but he soon became attracted to chemistry.

In 1852, Wöhler's magnetic influence drew him to Göttingen. In mentality, teacher and pupil probably had much in common: it is clear that Müller was born with chemist's fingers. He also attended lectures by Weber,

Sartorius von Waltershausen and Hausmann. He took his degree (Dr. Phil.) there early in 1853.

In the spring of 1853, on Wöhler's special recommendation, he went to Munich, under an engagement to become Liebig's assistant.* The Munich laboratory was then in course of construction. The engagement being only a provisional one, in the spring of 1854 he accepted an invitation, conveyed through Hofmann, to come to London as private assistant to Warren De la Rue to aid him in the investigation of Burmese naphtha (Rangoon tar). Warren De la Rue's laboratory was located in the factory of his firm, that of Thos. De la Rue and Co., Bunhill Row, London, E., the chief business of which was the manufacture of bank notes, playing cards and postage stamps. Warren De la Rue had been trained as a chemist and had worked under Hofmann but he was also devoted to astronomy and became celebrated on account of his lunar photographs; in fact, he was the first to make photography popular for astronomical purposes. He was assisted in this work by Müller, who, strangely enough, never became a photographer—perhaps he was too much the artist.

Association with such a personality as Warren De la Rue must have contributed greatly to the development of Müller's character. A man of extraordinary charm of appearance and suavity of manner, gifted with tact, a courtier and diplomatist of the highest order, full also of scientific enthusiasm, the unformed, simple young German—for such he was, it is said, when he came here—could not fail to be markedly influenced by daily contact with so alluring and stimulating a presence, the more so as he was also brought through De la Rue into the Hofmann circle, where he came into touch with men such as Peter Griess and Martius.

At this time he was in close touch with Kekulé and that their conversations were not without effect is clear from the following classical reference left on record by Kekulé:

"Während meines Aufenthaltes in London wohnte ich längere Zeit in Clapham Road in der Nähe des Common. Die Abende aber verbrachte ich vielfach bei meinem Freund Hugo Müller in Islington, dem entgegengesetzten Ende der Riesenstadt. Wir sprachen da von mancherlei, am meisten aber von unserer lieben Chemie. An einem schönen Sommertage fuhr ich wieder einmal mit dem letzten Omnibus durch die zu dieser Zeit öden Strassen der sonst so belebten Weltstadt, 'outside,' auf dem Dach des Omnibus wie immer. Ich versank in Träumereien. Da gaukelten vor meinen Augen die Atome. Ich hatte sie immer in Bewegung gesehen, jene kleine Wesen, aber es war mir nie gelungen, die Art ihrer Bewegung zu erlauschen. Heute sah ich, wie vielfach zwei kleinere sich zu Pärchen zusammenfügten; wie grössere zwei kleine umfassten, noch grössere drei und selbst vier der kleinen festhielten und wie sich Alles in wirbelndem Reigen drehte. Ich sah, wie grössere eine Reihe bildeten und nur an den Enden der Kette noch kleinere

* *Cf.* Liebig und Wöhler, 'Briefwechsel, 1829—1873,' vol. ii, p. 2.

mitschleppten. Ich sah, was Altmeister Kopp, mein hochverehrter Lehrer und Freund, in seiner 'Molecularwelt,' uns in so reizender Weise schildert; aber ich sah es lange vor ihm. Der Ruf des Conducteurs: 'Clapham Road' erweckte mich aus meinen Träumereien, aber ich verbrachte einen Theil der Nacht, um wenigstens Skizzen jener Traumgebilde zu Papier zu bringen. So entstand die Structurtheorie" ('Ber.,' 1890, vol. 23, p. 1306).

De la Rue was both man of the world and a scientific amateur of the highest type and it is testimony to the worthlessness of our public school system that such men have been so rarely produced during the past fifty years; the material from which they are made cannot have been exhausted. In early years, from 1870 onwards, when I was associated with the London Institution, De la Rue was the guiding influence and the active promoter of an interest in science among his fellow managers. He was assisted by Alfred Smee, of Smee battery fame, one of the early workers on the electro-deposition of metals, who, as physician to the Bank of England, lived close at hand in the Circus—a noteworthy character also as a lecturer, in a sometimes raucous, sometimes falsetto voice, on "My Garden"; the gardening cult was just then coming into fashion. There was no one to follow De la Rue, so after his death science had no supporter and the Institution itself gradually lapsed into decadence. In its new guise, as a home of Oriental studies, the best of work may be done in it but it will have no local influence on the City in promoting the spread of a knowledge and love of science and literature—an influence more necessary perhaps there than anywhere else in our country at the present day.

Grove's celebrated lectures on the correlation of the physical forces were originally delivered in the theatre of the London Institution. The inventor of the "Grove cell," he there made the fundamental discovery that water could be dissociated by platinum heated in the oxy-hydrogen flame and did his work on the hydrogen-oxygen gas battery in the rude laboratory behind the theatre, which was afterwards occupied by Wanklyn and then by myself. That no such quiet nook is now to be found in the City is very lamentable.

Being on the spot in Bunhill Row, Müller was often appealed to by the factory, with reciprocal advantage to both parties; he was eventually absorbed by it and threw himself into its work with utmost zeal, so satisfactorily that, after a time, he was made a partner; when the firm was converted into a limited company he became a director, retaining this office until his retirement in 1902. He may be regarded as having been the prototype of the modern industrial chemist. He was eminently successful and he ultimately became a very wealthy man; indeed, his means were far greater than those of any of the distinguished scientific men of his period who entered into association with industry.

One of his first tasks was to study the electrotyping process, which until then had been little used; after a considerable amount of experimental work he was able to make it technically trustworthy and an important branch of the firm's activities. Gradually, his attention became more and more

absorbed in the practical applications of chemistry, especially in the preparation of the colours and varnishes required in lithographic and colour-printing in general, even including the study of paper-making, in which he became an expert. There was no branch of the firm's practical work, in fact, with which he was not fully conversant and to which he did not contribute improvements.

Consequently, his time and attention were fully occupied and he was unable to carry on research work on his own account. Nevertheless, he remained an ardent student of his subject: having an encyclopædic mind, he was a systematic and critical reader of scientific journals covering a wide range of subject; he was also a regular attendant at all our scientific meetings. He therefore quietly took a leading part in the affairs of the Chemical Society and was always one of its most trusted advisers. He joined that Society in 1859; he held office as Foreign Secretary from 1869 to 1885; and he was President during the years 1885 and 1886. To mark his connexion with the Society and also with the Lawes Agricultural Trust, since his decease his widow and daughter have made handsome gifts to the two bodies.

He was President of the Mineralogical and Crystallographical Society of Great Britain during the period November, 1901, to November, 1904. He was elected a Fellow of the Royal Society in 1866.

Somewhat to the surprise of his friends, who had perhaps thought of him as too shy and too much the student ever to take on domestic responsibilities, in 1878 he married an Englishwoman, Miss Elizabeth Rusel Crosby, a friend of the De la Rue family. He then became a naturalised British subject.

Attracted by his great friend, the late Dr. Atkinson, lecturer on chemistry and physics at the Sandhurst Military Academy, well known as the author of the English edition of Ganot's 'Physics' and as an accomplished literary worker, who was resident at Camberley, Surrey, in 1885 he purchased an estate there and after building a house set to work to establish a garden; in course of years, with infinite care and labour, he converted a pine-covered, sandy waste into one of the most interesting and fruitful gardens in the country—on account of its contents rather than its arrangement, although it was full of beauty.

On his retirement in 1902 he became a worker in the Davy-Faraday Laboratory attached to the Royal Institution. During the greater part of the year he was regular in his attendance on five days in the week, spending only the week-end and the vacations in his garden at Camberley—notwithstanding the great attraction this had for him. No young worker could have been more assiduous or more *begeistert*, although below the surface, for he was a man who always kept his feelings to himself and was ever chary in his utterances. To watch his manipulation was to realise the spirit and methods of the past, characteristic of the great Liebig-Wöhler school, a spirit born of the love of pure discovery and joy in the exercise of the craftsman's skill. Always healthy and vigorous up to the day of his death, he never rested

from work; he was ever moved, in fact, by the purpose made manifest by Ulysses in Tennyson's lines:

> How dull it is to pause, to make an end,
> To rust unburnished, not to shine in use.

Just as in early life he had a remarkable command of chemistry, so in later life he developed a wonderful first-hand knowledge of trees, shrubs and plants. His main interest lay in growing them and knowing them, one after the other; in this he was greatly assisted and encouraged by friends, such as the late Director of Kew, Sir Wm. Thiselton-Dyer and the late Sir Michael Foster, with both of whom he was on terms of closest intimacy. Just as the Perkin family have worshipped new substances and spoken of them as beautiful, so he worshipped plants that were new to him and found beauty in them even when they were ugly—from the gardener's point of view. His "rock garden" was built at a time when rock gardens were not yet in vogue and we had not begun to emulate the Japanese in the worship of rocks; it had no pretensions to being anything but a place in which "rock plants" could be separately grown with advantage—built in pockets, under his own eye and to no small extent by himself, many of the plants in it were the produce of his own raising and constant loving care.

Most unfortunately, he had no literary gift: it bored him even to describe his experimental work; the accumulation of lore stored in his memory is consequently lost to us. It is hard that we have no means either of persuading or of compelling such men to disclose their soul's secrets. To think what he could have told us outside ordinary well-known things: of materials, of processes, of plants and their ways; nothing is more disappointing than the manner in which we allow real knowledge, hardly won, to be sacrificed; vast as is the output of books at the present day, they are of little if any value—for the most part vain repetition; the things that count in practice are too rarely recorded by workers: if we can take an interest in bees and wonder at their ways, surely we should delve more deeply into the doings of the best of our own kind.

Behind the scenes, on account of his experience and judicial temperament, Hugo Müller more than once played an effective part as an intermediary. Owing to his friendship with Caro as well as with Perkin, in the early days of artificial alizarin, he was instrumental in bringing about an understanding between the English and German interests. His services were again called into requisition, with advantage, I believe, when difficulties arose over the transfer of Perkin's business to Messrs. Brooke, Simpson, and Spiller.

Evidence that Hugo Müller was thoroughly schooled as an analyst and a close observer, even in his youth, is to be found in his early mineralogical notices. The first of these, published in 1852 in the 'Correspondenz-Blatt des Zoologisch-mineralogischen Vereins zu Regensburg,' was referred to at length by Erdmann in the 'Journal für praktische Chemie' as the work of Hugo Müller, Stud. Phil., Göttingen; it contains an account of several rare

minerals: Beryll, Columbite and Rutile, found at Tirschenreuth, his native home. Erdmann, in a footnote, refers to a correspondence with the author in which the latter speaks of having been in doubt as to the accuracy of some of his results at the time of putting them forward, stating that he had been able subsequently to confirm them.

Several years later, in 1859, he published full analyses of a meteoric iron from Mexico and of a number of minerals of interest. While with Liebig he worked out a method of preparing lithium salts from the mineral Triphyllin found in a coarse granite at Rabenstein, in the Bavarian Forest; he refers to this as a most suitable material on account of the high percentage of lithia and its solubility in acids. Liebig apparently had contemplated the use of lithia in the manufacture of an easily fusible optical glass but nothing came of this idea.* Lithium carbonate was quietly brought into use in medicine as a solvent of uric acid, however. The following reference to this work by Stas is of interest:

"Pour obtenir du chlorure de lithium, je suis parti du carbonate de ce métal. Je dois une partie de ce carbonate à l'amitié de M. le docteur Hugo Müller, dont les beaux travaux sur les composés de lithium sont connus de tous les chimistes. . . . Le carbonate que je devais à l'obligeance de M. Hugo Müller et celui que j'avais extrait moi-même de la triphylline étaient déjà assez purs et ils ne contenaient que des traces de sodium et de calcium" (Stas, 'Œuvres Complètes, vol. i, pp. 689–690).

Müller appears to have attracted Wöhler's particular notice and at his suggestion undertook the study of the then little known compounds of palladium with ammonia. An account of the work was published in 'Liebig's Annalen' under the heading: "Palladamine, von Hugo Müller, Aus dessen Inaugural Dissertation, Göttingen, 1853." It was known that palladous chloride and ammonia gave rise to two characteristic compounds, one red, the other yellow, also that these were of the same empirical composition; Müller's task was to discover the nature of the relationship. Struck by the fact that the yellow salt could even be heated with caustic alkali without ammonia being evolved, guided evidently by Reisset's then recent work on the platinum bases, he came to the conclusion that the compound was an ammonium salt in which palladium displaced hydrogen, $N(Pd'H_3)Cl$; he assumed that the red salt, the precursor of the yellow, was simply a compound of ammonia with palladous chloride and that the reason why ammonia was not discharged from it by alkali was that it underwent conversion into the yellow salt on such treatment. He prepared a series of salts and also the base in a crystalline state and showed that the latter was far stronger than ammonia. He also described the palladodiammine salts formed by the action of ammonia in excess. Ethylamine and aniline were shown to give compounds corresponding with those formed by ammonia.

It is strange that, to the present day, compounds of ammonia such as were

* Liebig und Wöhler, 'Briefwechsel,' 1829—1873, vol. ii, p. 10.

studied by Müller in 1852, of which so large a number are known, must still be ranked among the unguessed riddles of chemistry; we know no more than he did of their constitution. Dazzled by the practical results of Werner's labours, there has been a tendency of late years to assume with this chemist that they may be represented as compounds in which the linking element (the metallic atom) is of high valency. But Werner's so-called co-ordination formulæ are mere mnemonic expressions of quite limited application, indicative of certain very obvious properties but in no way acceptable as structural expressions. The loss of individuality suffered by the constituents of such compounds on combination is indicative of some form of union comparable with that manifest in so-called closed-chain (benzenoid) carbon compounds. Methods of determining the internal structure of crystals with the aid of X-rays such as are now coming into vogue should be of special service in connection with such compounds.

It is worth noting how much Hugo Müller had achieved in four years, under the wise system of practical training of those early days, at the close of his *Wanderjahren* as a student. It is clear that he was already an accomplished independent worker—he had been allowed to develop freely, not crammed, as are our students, too often to be mere dictionaries of undigested facts with pages stuck together so that they cannot be read for any useful purpose. Our talk of reconstruction at the present day, of the great things we are going to do in science, will come to nothing unless we can free ourselves from the curse of examinations and prize-hunting—unless we can develop a free system of training comparable with that which gave such wonderful results in the early and middle period of last century: students were then allowed to help themselves and to take the consequences if they did not; science was the pursuit of persons of independent character and the drones were few. Little wonder that men learnt to think and to be observant and critical. In those days, too, there were great teachers, men of courage and individuality, whose one object was the unselfish pursuit of truth. It is the loss of these we have most to lament: the lowering of ideals; the fall in our social standards; the lack of independence consequent on the struggle for place and the concomitant desire for advertisement. Science can have little success in our country, in the near future, unless it be assured of a more healthy environment.

Hugo Müller's first work in this country, the investigation of Burmese naphtha (Rangoon tar), was carried out in conjunction with Warren De la Rue. Incomplete and preliminary as is the only published account, it is of special interest because so many of the practices of to-day must be traced back to it. On distilling the naphtha in ordinary and in superheated steam, all but about 4 per cent. was volatilised; from one-tenth to one-third of the more volatile fractions were removable by sulphuric acid. Of special interest but long overlooked is the discovery they made that these fractions were rich in benzene, toluene, xylene and cumene. These hydrocarbons were not

isolated—the methods of separating them were not then known—but their presence was inferred from the production of nitrobenzene, of dinitrotoluene, of trinitroxylene and of barium sulphocumolate.

In a later communication they state that while pursuing the study of the naphtha they had noticed among the products of the action of nitric acid *on certain liquid hydrocarbons* contained in the oil "a peculiar acid." They add, "A very lengthened investigation of this acid and its derivatives we are about bringing to a close but as the drawing up of this account will necessarily occupy a considerable time, we have thought it desirable to send a short abstract of the chief results we have obtained." It was ever so—all Müller's early work is in the form of incomplete short accounts: following the fashion of the day, his enthusiasm led him to unburden his mind of his secrets in preliminary notices but he had no desire to advertise at length. The bad habit of reproducing illogically ordered note-books was not then established.

The "peculiar acid" was identified with the terephthalic acid first obtained by Caillot, fifteen years earlier, by oxidising turpentine, also with Hofmann's insolinic acid obtained in a similar manner from oil of cumin (cuminaldehyde). The methylic salt was described as the most characteristic derivative of the acid, together with various other derivatives, including nitro-, amino- and hydroxy-terephthalic acids.

De la Rue and Müller were led by this work to oxidise coal-tar naphtha but the results reported were not very definite; in those days the benzenes were still mysterious substances.

A little later, Müller was led to study phthalic acid in continuation of his work on the para-isomeride. He gave a very full account of its properties as compared with those of terephthalic acid in a purely preliminary notice only two and a half pages long.

De la Rue and Müller also studied rhubarb, confirming previous observations and isolating emodin for the first time in preparing chrysophane; years later he stated that he had also isolated alizarin.

The resin of *Ficus rubiginosa* from New South Wales also engaged their attention, the material having been obtained from the Paris Exhibition of 1855. They examined the resin, the chief constituent, with no very definite results. In addition, they isolated from it a crystalline acetate—cycoceryl acetate—and prepared from it an alcohol of the formula $C_{18}H_{30}O$, which they regarded as a member of the benzylic series. Obviously they were curious people with eyes open in various directions. Noting the spontaneous decomposition of a quantity of nitroglycerin—they do not say why they had such a substance on hand—they investigated the product and obtained the crystalline barium salt of an acid which they subsequently identified, on the appearance of Debus and Sokoloff's account of glyceric acid, with this acid.

Hugo Müller was one of the first to undertake the purification of coal-tar phenol. For this purpose he exposed an alkaline solution of the crude tar acid to the air and when the oxidation of impurities was complete treated the liquid fractionally with acid. The oil separated by one-sixth of the

required total acid carried down impurities formed by oxidation, the second sixth liberated mainly the less acid cresols; phenol was then prepared from the remainder by fractional distillation. The use of lead oxide as a means of removing sulphur compounds from the phenol is described in the paper, I believe for the first time.

He also studied wood-tar and confirmed Hlasivetz's observations on creosole, $C_8H_{10}O_2$, showing, in addition, that when distilled with hydrogen iodide this afforded methylic iodide. This is a very early, if not the first, use of the iodide as a demethylating agent.

In purifying crude phenol his attention was attracted to rosolic acid, which had been unnoticed since its discovery by Runge. In this connexion reference may be made to one of his most interesting discoveries, that of hydrocyanorosaniline, formed as a colourless cystalline precipitate on adding potassium cyanide to a solution of a rosaniline salt. His acumen led him to recognise it not as a salt but as a base similar to leucaniline.

In 1866 Stenhouse and he described the preparation of picric ether and also an improved method of making chrysammic acid from Socotrine aloes. It was suggested that the acid might be "tetranitrochrysophane" but at that time it was not recognised as an anthracene derivative.

But his most important early work, which gave him his reputation, was that on the use of iodine as a carrier in effecting chlorination, as the use of such catalysts must be dated back to this work. In studying the hydrocarbons of Burmese naphtha, De la Rue and he had tried the action of the chlorides of iodine, in the hope of obtaining iodine derivatives; they found that only chloro-compounds were formed, such as were produced by chlorine alone; they noticed, however, that chlorination was more readily effected in the presence of iodine.

Desiring to prepare the alcohol and acid corresponding with benzene, Müller was led to study the action of chlorine on the hydrocarbon; having had the experience referred to, he added iodine. He was thus led to the discovery that, whereas benzene chiefly *combined* with chlorine when the two substances were brought directly together, in presence of iodine it was rapidly converted into the substitution derivatives mono-, di- and tri-chlorobenzene.

On making experiments with other compounds it was found that iodine had a remarkable general effect in facilitating the action of chlorine. He also drew attention to the similar effect produced by other intermediary agents such as antimony pentachloride.

His further development of the discovery was logical. He first applied it to the preparation of chloracetic acid—now a substance of great importance in the manufacture of artificial indigo; subsequently, he extended the use of iodine to the preparation of dichloracetic acid. And having chloracetic acid at his disposal, he proceeded to prepare cyanacetic acid and converted this by hydrolysis into malonic acid; here again he was rendering a marked service, as malonic acid, at a later date, became of great importance as a synthetic agent. He evidently intended to extend the inquiry, as he had

prepared chloropropionic acid from lactic acid and through the cyano-derivative had obtained an acid which he thought might be succinic.

Müller's communication to the Chemical Society is printed next to a short notice by Kolbe describing the same two discoveries; apparently, the two workers had agreed that only one of them should continue the inquiry. It is significant that in those days lactic acid was so little understood that neither realised that it could not give rise to succinic acid. A year or two later the dibasic acid prepared from it was studied in Kolbe's laboratory and shown to be *iso*succinic acid.

A branch of Müller's work known to but few was that relating to paper-making materials. So much was he regarded as an authority on this subject that he was called upon to write the official report on this section of the chemical exhibits at the Vienna International Exhibition of 1873. It was issued in book form in 1876 under the title 'Die Pflanzenfaser und ihre Aufbereitung für die Technik' (149 pp.). The book is full of lore and technical information but not chemical for the most part. The elegant method which he devised for the estimation of cellulose in fibres is described, however; this involves the alternate action of bromine and ammonia repeated until the former no longer has any effect. It may be mentioned, as an indication of progress, that in discussing the production of wood pulp, practically the only process described is that involving disintegration by alkali; the modern sulphite method is not referred to.

Warren De la Rue and his assistant entered on a new line of work in 1868, when they devised their well-known chloride of silver non-polarisable voltaic element of constant E.M.F., consisting of silver chloride fused around a silver wire opposed to zinc in a solution of ammonium chloride, each cell being of small dimensions and in tubular form. By associating such cells in very large numbers—up to 11,000—they were able to produce a battery of high E.M.F. such as had never before been contemplated as possible. The form given to the battery was an exemplar of the two men, showing the grace of the one and the deftness of the other. Each cell had an E.M.F. of 1·03 volts; it may be mentioned that the voltage of the corresponding silver bromide cell is 0·908 and that of a silver iodide cell 0·758.

A most important series of experiments, which did not attract the attention they deserved at the time, were made by Bleekrode in 1877, in De la Rue's laboratory, to test the behaviour of various binary compounds at high voltages. He was able to show that liquids such as carbon disulphide, benzene, cyanogen, zinc ethyl, tin tetrachloride and hydrogen chloride were not in the least affected. It is interesting that, in referring to these observation in 1877, De la Rue and Müller pointed out, that "it is most remarkable that fused chloride of lithium is readily decomposed by four Bunsen cells and fluid hydrochloric acid resists a potential of 5640 cells, the accepted notions of electrolysis evidently requiring consideration."

During the years 1877–1883 De la Rue and Müller described in great

detail, in four papers published in the 'Philosophical Transactions,' the observations they had made on the electric discharge in air and in other gases and in exhausted tubes. At the time these excited much attention, as they were brought prominently under notice; probably they contributed, in no small degree, to excite an interest in such studies, to judge from the activity with which research was pursued soon afterwards in this country into the problems of the electrical discharge. But the phenomena observed were too complex to meet with any direct application or interpretation at that time.

On taking up scientific work, after his retirement from business, Hugo Müller naturally bethought himself of problems brought under his notice in earlier days. When collecting materials for his article on vegetable fibres in A. W. Hofmann's report on chemistry at the Vienna Exhibition of 1873, he had occasion to examine the leaves of the palm *Chamærops humilis*, then imported from Spain as a paper-making material. He had found in the dried leaves as much as 1·35 per cent. of dextroquercitol, a substance previously met with only in the acorn. It may be noted that Power and Tutin, in 1904, were able to isolate lævoquercitol from the leaves of *Gymnema sylvestre*.

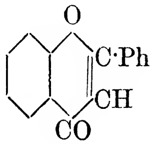
On extending his observations, he had found in leaves of *Cocos plumosa* obtained from Kew and in dried leaves of *Cocos nucifera* (the ordinary coconut palm), from Jamaica, not quercitol but a new substance, *Cocosite*. When he resumed laboratory work in 1903, he set out to investigate this compound but had the greatest difficulty in obtaining any quantity to work with, as palm leaves imported from various places, Barbadoes, Calcutta and the Seychelles, contained the material, if at all, only in traces. He again found it in the leaves of *C. plumosa*, grown at Kew; he separated a few centigrammes from a litre of coconut milk but could not find it in the perisperm. Much time was occupied in this preliminary search and he did not publish any account of the work until 1907. He then described *Cocositol* very fully and also several of the derivatives, showing that it was an isomeride of inositol or hexahydroxyhexahydrobenzene.

In view of the difficulty of procuring cocositol, he took up the study of inositol, of which he had extracted a considerable amount from cochineal. He studied particularly the action of hydrogen bromide on an acetic acid solution of the hexacetate. He prepared several bromacetins and the corresponding bromhydrols and subjected these to reduction.

In 1912 he gave a further and final account of his work and stated that he had identified cocositol with *Scyllite* isolated by Staedeler and Frerichs in 1858 from the kidneys and other organs of certain plagiostomous fishes and with the *Quercine* separated by Vincent and Delachanal in 1887 from acorns. He therefore withdrew his name in favour of *Scyllitol*. The most important result arrived at in this work was that under the influence of the halogen hydrides both scyllitol and inositol ultimately gave rise to the same products. He also described two new modifications of inositol.

He finally turned his attention to the peculiar characteristic pulverulent

surface deposit characteristic of many species of Primula and made the important discovery that it was identical with Flavone,

$C_{15}H_{10}O_2$ = 

synthesised by Kostanecki and Feuerstein in 1898. Flavone is resolved by hydrolysis into salicylic acid and acetophenone. The compound had not previously been obtained from any natural source, although several of its derivatives are characteristic, yellow, vegetable colouring matters, Flavone itself being colourless.

The three investigations are noteworthy proofs of his great command of technique, as he had only small quantities of material at his disposal in several cases and was called on to unravel complex mixtures of substances not at all easily separated. Shortly before his death he had begun to examine several substances which he had extracted in early days from ebony wood but no record is left of the work.

Hugo Müller died at Camberley on May 23, 1915. His decease was sudden and unattended with suffering; it was quite unforeseen and unexpected, although those of us who happened to see him at work a few days before had been much struck by his altered manner and his hopeless despair of the future: he died of broken heart, if man can so end his life.

Such is the history, so far as I can read it, of a typical German, a south German, of the old school—of a school that may be said to have served all nations and to have been welcomed by all in the past; ever courteous, conscientious to an extraordinary degree, unsparing of effort, always anxious to learn and progressive, although perhaps without great gift of originality.

H. E. A.

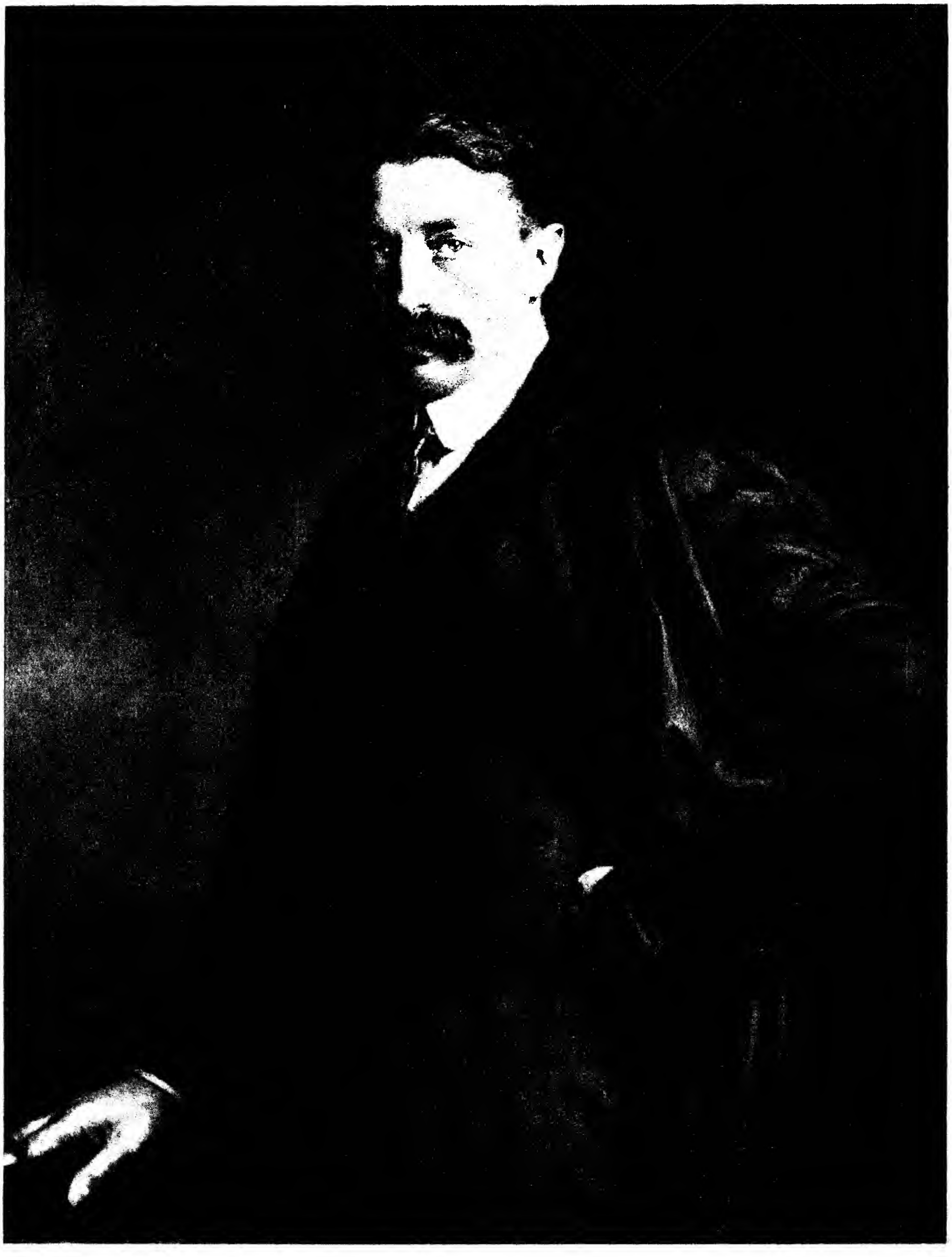
BERTRAM HOPKINSON, 1874–1918.

BERTRAM HOPKINSON, who was killed in a flying accident on August 26, 1918, while engaged on national service, was born at Woodlea, Birmingham, on January 11, 1874. He was the eldest child of Dr. John Hopkinson, F.R.S., born when his father, then only 25 years old, was at the threshold of a career which was to achieve great things in science. At that time John Hopkinson, but lately Senior Wrangler and Smith's Prizeman, had accepted a responsible position with Messrs. Chance Brothers, and was applying his scientific skill to the task of improving the quality of their optical glass and developing its uses, especially for lighthouse illumination. The position enabled him to marry: his wife was Evelyn Oldenbourg, a lady of remarkable gifts, to whom he had become engaged almost immediately after he left Cambridge. Bertram was the eldest of a family of six, three boys and three girls. When he was three years old the family moved to London, where John Hopkinson took up professional life as a consulting engineer and inventor. This he pursued with conspicuous success until his premature death by an Alpine accident in 1898.

Bertram owed to his parents far more than a heredity which was at once sound and of brilliant promise. The home atmosphere could not have been more congenial or more stimulating for a boy born with scientific tastes and richly endowed with mental and physical activity.

John Hopkinson's absorption in invention and discovery never stood in the way of companionship with his children. He was in a rare degree his children's friend, and to Bertram especially this friendship was in itself a liberal education. The boy was no doubt extraordinarily receptive. From early childhood he was the almost constant associate of his father, sometimes assisting him in work, often his comrade in play. He lived at home, going as a day-boy to St. Paul's School, and enjoying intensely his father's company in the hours spent out of school, joining in long walks, in discussions, in experiments. Always strong in body and mind, he could profit by such a life without being over-strained. He prospered at school, was the youngest boy in his mathematical form, and secured a major scholarship at Trinity before he was seventeen. By that time he had imbibed not only much science and engineering, but the habit of scientific thinking and of applying scientific thought to real problems. In the researches he was to publish later, one sees at every turn the reflection of his father's mind. From his father, too, and scarcely less from his mother, he acquired a wholesome love (which he never lost) for most kinds of open-air activity. Walking, climbing, rowing, sailing, skiing, chamois-hunting in the Tirol, in such things he spent his holidays with the keen pleasure of one who was immensely alive in everything he undertook.

At Cambridge he read for the Mathematical Tripos, which by that time was divided into a first and second part. In the first part he was compelled



B. Hopkinson

to content himself with an ægrotat degree. An unlucky attack of illness—a rare event with one so robust—prevented his taking the examination; but next year, in the second part, his quality was shown by his being placed in the First Division of the First Class. For the rest his University life was normal and uneventful. He made some lasting friendships, and was one of a Trinity crew at Henley.

On leaving the University at the age of 22 he proceeded to read for the Bar, a profession with which his father was connected by often serving as an expert in patent actions, and in which Bertram could count upon useful introductions. After working in counsel's chambers, where he was soon recognised as a pupil of distinction, he was called to the Bar in 1897. Next year he was on his way to Australia to carry out a legal enquiry, when the tragic death of his father, his brother, and two sisters changed the course of his life. The family were spending their holiday that autumn in Switzerland, at Arolla, where Bertram joined them for a few days before taking ship to Australia. The days were spent in climbing expeditions, which included a traverse of the Matterhorn, after which Bertram went off to pursue his journey, and his father returned to Arolla. A few days later, John Hopkinson, with three of his children, was killed in climbing the Petite Dent de Veisivi. A telegram recalling Bertram reached him at Aden.

Faced with this calamity, Hopkinson decided to take up, so far as he could, his father's unfinished work. He joined in partnership with his uncle, Mr. Charles Hopkinson, and his father's valued assistant, Mr. Talbot, and with them, was responsible for the design of electric tramways at Crewe, Newcastle, and Leeds, as well as other works. To do this required courage, not to say daring: in that he was never wanting. It required, too, a rare adaptability, and perhaps was possible only because of the irregular and almost unconscious training in engineering which he had received from his father. An important element in his life at the time was his intercourse with Sir Benjamin Baker, who had been for many years a close friend of the family. Sir Benjamin appreciated Bertram, enjoyed scientific discussion with him, and remained, until his death in 1907, an intimate and most helpful friend.

Hopkinson not only succeeded in the practical aspect of his new undertaking, but soon found in it material for scientific research. A paper by the three partners on "Electric Tramways," read before the Institution of Civil Engineers in 1902, gives particulars of his experiments on the electrolysis of pipes and other matters arising out of their engineering work. For this he was awarded a Watt Gold Medal by the Institution, of which later he became a full member.

In 1903 the chair of Mechanism and Applied Mechanics in the University of Cambridge became vacant. Hopkinson was then only 29 years of age, and so far as teaching was concerned he was wholly untried. But already he had a considerable professional reputation, and he gave a personal impression of brilliancy and promise which satisfied the electors. In

electing him they certainly made a wise choice, and were entirely justified in the result. The professorship meant his assuming charge of the Cambridge Engineering School, which by that time had become an active and considerable section of the University. Its expansion had been rapid from about 1892, when the Mechanical Sciences Tripos was established. Successive extensions of the buildings had been made which barely kept pace with its growth in numbers. One of these extensions was the Hopkinson Wing, erected in memory of John Hopkinson and his son John by the surviving members of the family. There was a peculiar appropriateness in Bertram's appointment to be head of an establishment in whose creation his father had from the first taken a helpful interest, and whose development was in this way associated with his father's name. Under Bertram's leadership the prosperity of the Engineering School was more than maintained. It continued to grow in numbers and in academic and professional repute. Its attractiveness to students was in some sense a danger, but he was careful to fence the approach to the Tripos so effectively as to maintain a high standard. His own passion for research found in Cambridge a wider scope than had been open to it in the earlier part of his career. He entered there upon a period of remarkable productivity, stimulating selected students to attempt original work, and securing their co-operation in many important researches. He became a Fellow of the Royal Society in 1910, and was serving on the Council at the time of his death.

His marriage, in 1903, almost coincided with the beginning of his professoriate. His wife, who was the eldest daughter of Mr. Alexander Siemens, survives him with seven daughters. In domestic life he found continuous quiet happiness, as well as freedom to pursue his scientific interests. There was no son who might have had from Bertram the same kind of nursing in scientific method that Bertram had from John. But if he had no son he had among his students not a few disciples who were fired by his teaching and example and were full of affection for their chief. Some of them will carry on the work he left undone. Others gave their lives, as he did, in the war. Of that band was his own brother Cecil, a man of rare promise, who passed out with First Class Honours in the Mechanical Sciences Tripos only a year before the war began. He had been his brother's assistant in more than one research. Cecil Hopkinson hastened to join the army in August, 1914, and received a fatal wound in Flanders near the end of the following year. Those who knew Cecil and his great potentialities felt the eclipse of that young life to be far more than a personal loss. To Bertram it was a grievous blow; but he did not let it disturb his absorption in war work, which by that time was complete.

During his tenure of the Cambridge professorship, Hopkinson took no large part in the administrative work of the University outside of his own department, the claims of which were sufficiently exacting. He was, however, an energetic promoter of the Officers' Training Corps, and organised in it an engineering section, thereby maintaining a family tradition; for his father

had been a pioneer in the foundation of the Corps of Electrical Engineers. In 1914 he accepted a professorial fellowship offered him by King's College. Outside the University he took a leading part in the work of more than one Research Committee. He served jointly with Sir Dugald Clerk as Secretary of the British Association Committee on Gaseous Explosions, whose Reports, spread over several years, contain records of many of his own experiments. He was an original member of the Advisory Committee set up in connection with the establishment of the Department of Scientific and Industrial Research. When the Royal Society appointed a committee of engineering experts to advise on problems of the war, Hopkinson became its secretary. The indirect effect of this was specially important; it brought him into close relations with the technical experts of the Army and Navy, and so did much to determine the direction which his energies took in the final years when his undivided efforts were given to national service.

On the outbreak of the war he dropped all other interests. Obtaining a commission in the Royal Engineers he first undertook teaching duty at Chatham to relieve others for active service. Later he was engaged at the Admiralty in a department organised by the present writer, on work of a kind entirely new to him, which he took up with conspicuously good effect. It was mainly concerned with the collection of intelligence; but independently of that, he was at the same time occupied in conducting experiments in connection with an arrangement for the protection of warships from the effects of mines and torpedoes, by the addition to the hull of a "blister" or outer shell. His work showed, by experiments of gradually increasing scale, which were most ably carried out, that the law of comparison held good, subject to a certain modification in going to full size, and he suggested the insertion in the "blister" of a structure capable of absorbing the energy of an explosion in the act of becoming deformed, in lieu of a water-jacket. The value of his contribution was quickly recognised by the Admiralty; it at once received official acceptance and substantial acknowledgment. It has been applied in the design of some of the newest units of the Fleet, and is a feature of one of the most powerful of them, the battle cruiser "Hood." The origin of the suggestion is of interest, for it arose out of an earlier scientific research which Hopkinson had carried out with no idea at the time of putting it to this important use. For years before the war he had made a special study of explosions, their nature and the measurement of their effects. The events of 1914 gave this kind of knowledge a new significance. He applied himself to problems of attack as well as of defence, taking up not only the protection of ships, but the design of bombs for use by air-craft. From that he passed to the equipment of air-craft generally. He accepted a position under the Air Board, and in each successive transformation of that branch of the Service the authority and responsibility of his office seemed to be enlarged. Some months before his death he had become a colonel and was awarded the C.M.G.

Much of his work was done at an experimental station on the East

Coast, but he had to visit many aerodromes, and he found that flying was his best means of locomotion. It was, moreover, an art he had to acquire in the interests of the work itself. He soon learnt to be his own pilot, and generally flew alone. It was in one of these flights that he fell, near London, in bad weather on August 26, and was instantly killed. The Air Council took the unusual step of recording "their deep sense of the high and permanent value of the work done for the Flying Forces by the late Colonel Hopkinson, and their recognition of the private self-abnegation with which he devoted his great abilities and scientific attainments to the public service"; and they communicated to the Vice-Chancellor of the University "an expression of profound regret at his untimely death and at the loss which has thereby fallen on the University of Cambridge."

Of Hopkinson's work for the Air Force, the following notes have been furnished by one who served under him in it, Lieut.-Col. H. T. Tizard:—

"His work in connection with flying began about March, 1915, when he was asked by the Department of Military Aeronautics to conduct experiments on bombs. These were carried out at Cambridge and elsewhere, and included the building of a model factory on a one-sixth linear scale on which the effect of model bombs was tested with the object of determining (*a*) the best proportion of bomb-case to weight of explosive, and (*b*) the best material of which to make the case. The experiments were continued until the end of May, 1915, when the model building was completed and the trials were witnessed by the Ordnance Board and representatives of other Government departments. While preparations for these trials were proceeding, he was consulted by the Superintendent of the Royal Aircraft Establishment in connection with the design of bomb-sights. In July, 1915, he was appointed on the panel of Lord Fisher's Board of Invention and Research, and during the following months until November, his time was chiefly occupied in work for that Board, and in other experiments in connection with bombs and gyro bomb-sights both and at Cambridge and at Farnborough.

"In November an official connection with the Royal Flying Corps was established by his appointment to the Department of Military Aeronautics, where he took charge of both the design and supply of bombs, bomb gears, guns, and ammunition. Most of the experimental work for the corps was at this time carried out either at manufacturers' works or at the Central Flying School, Upavon, and at Farnborough and Hythe. It was not long before Prof. Hopkinson saw the drawbacks of combining experimental and training work at one station, and he urged the formation of a separate station for the former. This recommendation was adopted, and an armament experimental station was started at Orfordness in the spring of 1916. The work of this station was entirely under the control of Prof. Hopkinson, and he threw his whole energies into its development. Most of the personnel was selected by him from existing R.F.C. stations. By the middle of 1916, in spite of the difficulties of working in temporary buildings, the station was in full swing. Shortly afterwards a large amount of the supply work,

which had hitherto been done by Prof. Hopkinson's section at headquarters, was transferred to a special supply section, which left him more time to devote to purely experimental work. The work at Orfordness had great influence on the development of armament in the R.F.C. and subsequently in the Air Force, and was very varied in character. It included the development of bombs, bomb-sights and methods of bombing; guns, gun-sights, and ammunition; self-sealing tanks and other accessories; and not least the systematic development of night flying, and of navigation in clouds and in bad weather, the influence of which work was beginning to be felt strongly at the time of his death, and will increase as time goes on. In all this work Major Hopkinson was at his best. He possessed the great capacity of understanding human limitations and knowing where, for war purposes, it was uneconomical to proceed further with the development of any particular scientific work. His judgment was well shown in the great pains he took to keep training and experiment in the closest possible contact.

"The success of the work at Orfordness was such that it was soon decided to put the testing of aeroplanes, which up to this time had been done at the Central Flying School, also under his direction. Towards the end of 1916 this work was removed to Martlesham Heath. Major Hopkinson selected the site and made all preliminary preparations for clearing it and putting buildings in hand. The station soon developed in importance, and the work, which consisted of the testing of complete aeroplanes, of engines and other accessories as well as a certain amount of experimental photography, expanded considerably under his direction.

"At the end of 1917 Lord Rothermere's reorganisation of the Air Force still further increased the scope of Major Hopkinson's duties. The experimental work at the R.A.E. and the Naval Aircraft Experimental Stations at Grain, came under his control, and from this time until his death his influence on the general development of aeronautics steadily increased. He was appointed Deputy Controller of the Technical Department in June, 1918.

"He found time, in spite of the pressure of his work, to learn to fly at Orfordness. There can be no doubt of the wisdom of this step, although many of his friends thought that it was an unnecessary risk. It increased both his judgment and his influence, especially his influence over the officers at the experimental stations. He took an intense pleasure in flying. He learned remarkably quickly for a man of his age, and was soon at home on many types of machines. At the time of his death he was flying a Bristol Fighter, and had started from Martlesham Heath on his way to London. The weather was threatening at the start, although the sky was only partly clouded, but in the neighbourhood of London the conditions were much worse and the sky was completely covered with low clouds. It seems clear that he flew above the clouds for some way, and then finding no gap descended through them. He probably lost control of the machine in the clouds (which would be quite easy for even a very experienced pilot to do on the type of machine he was flying), and on going through the clouds did

not have sufficient time to regain control before the machine crashed, and he was instantaneously killed."

It may be added that immediately before his death plans were being matured for the establishment of a great national school of Aeronautical Engineering, of which Hopkinson was to have been the head.

Hopkinson's published work included a memoir of his father, written as a preface to the reprint of John Hopkinson's collected papers, as well as many accounts of his own researches. No attempt need be made here to analyse these, but a rough notion may be conveyed of the scope of the more important of them. For this purpose they may be grouped under three or four general heads.

Two papers, written in conjunction with Sir Robert Hadfield, describe researches on the magnetic properties of alloys. The molecular theory of magnetism which ascribes the process of magnetisation to the turning into alignment of molecular magnets whose moment is constant, implies that there is a finite "saturation" limit to the magnetisation that can be reached under the influence of any magnetising, however great. According to the present writer's molecular theory, this limit is easily reached, for the molecular magnets are free to turn save for the control they exert on one another through their mutual magnetic forces. Experiments made as early as 1887 by the "isthmus" method had shown that this is the case in iron, and had determined the saturation limit. Hopkinson, adopting the "isthmus" method, confirmed this conclusion, and went on to examine the limit in a series of iron alloys prepared by Hadfield. These were steels containing various percentages of carbon, and the results confirmed Hopkinson's anticipation that the magnetism of saturation might be predicted from the composition, treating each steel as a mixture of iron and of non-magnetisable carbide of iron. This deduction should follow from the principle (deduced from the molecular theory) that the saturation magnetism of an alloy is the sum of that of the constituents, when due account is taken of the proportions in which they are present, provided they behave simply as a mixture and do not interfere with one another's magnetic properties. It was found that this principle held good in carbon steels with sufficient exactness to suggest that measurements of magnetism might be used as a means of determining the proportion of carbon present in the form of carbide of iron. It was similarly found that silicon and aluminium act mainly (in the magnetic sense) as inactive diluents. With manganese no such simple relation was discovered, and the anomalies which that metal presents in association with iron are suggestive in connection with the Heussler alloys, where it acts as one of three constituents, each non-magnetic when tested alone, to produce an alloy that has a strongly marked magnetic quality. Another joint paper, published by the Iron and Steel Institute in 1914, describes an interesting investigation of the very complicated magnetic properties of Hadfield's manganese steel, which can be toughened by sudden cooling from a high temperature, in which state it is non-magnetic, but can be made to assume

various degrees of magnetic susceptibility by prolonged exposure to moderate heat.

Another group of papers deals with the elastic properties of steel and other metals and the departure from perfect elasticity which is known as elastic hysteresis ('Roy. Soc. Proc.,' A, vols. 76, 86, and 87). In the course of the investigation Hopkinson devised a high-speed fatigue-tester for examining the endurance of metals under alternating stresses of great frequency. With this machine he could reverse the stress in the specimen with perfect regularity as often as 7000 times per minute, measure the amount of energy dissipated internally through elastic hysteresis, and determine the number of repetitions that were required to produce fracture for various ranges of stress, as well as the range that could (apparently) be endured without fracture, no matter how often the reversal of stress might be repeated. Apart from the interest of the results, the methods of observation described in these papers have much individuality, and may be expected to open the way to further discovery in the same field.

The development of the gas engine and other internal combustion motors appealed strongly to Hopkinson as a practical matter on which scientific consideration could usefully be brought to bear. He invented a method of internal cooling, of which great things were hoped, but the results were disappointing. He also invented an optical indicator, which proved itself to be a most effective instrument for revealing what happens in the cylinder. He investigated problems of heat flow and temperature distribution, and he gave much time to the study of gaseous explosions by means of experiments in which a mixture of gas and air was ignited in a closed vessel furnished with appliances for exhibiting and recording the action at various points in the interior, from which the true nature of the action and the manner in which the explosion was communicated from point to point could be inferred. The devices used in this research were of great ingenuity, and the experiments were prosecuted with conspicuous thoroughness. They cleared up matters that had been obscure, and removed a number of current misconceptions. Sir Dugald Clerk has been good enough to furnish the following notes regarding Hopkinson's labours in this field:—

"The science of flame and explosion has suffered a great loss by the death of Bertram Hopkinson. He was one of the most brilliant and enthusiastic experimenters in that field, and carried out very important investigations into the phenomena of gaseous explosion by which he arrived at valuable and interesting conclusions. He demonstrated that in gaseous explosions the maximum temperature is attained in the space surrounding the ignition point. This is due to the adiabatic compression of the flame first formed at the sparking position, by the compression due to the rise in pressure produced by the inflammation of the outer layers, and the temperature there may be raised as much as 300° C. above the average of the explosion from that cause alone.

"He also proved by the use of platinum wire resistance thermometers that

in such explosions within closed vessels, the expanding flame fills the whole vessel considerably before the termination of the rise in pressure. This had been inferred in early explosion experiments by other experimenters, but the inference depended on a study of the changes occurring in the rate of rise of the explosion curve; it remained for Hopkinson to prove definitely by resistance thermometer observations that the flame reached the sides of the vessel in advance of the point of maximum explosion pressure.

" Hopkinson also determined the effect of radiation from the flame in checking the rise of pressure and in altering the rate of cooling. His first experiments were made with cylindrical vessels silver-plated internally and highly polished. In these vessels the maximum pressure increased about 4 lbs. per square inch, equivalent to a temperature rise of 70° C., in the polished vessels as compared with experiments with the same mixtures in the same vessels with the polished surface blackened over.

" To study further the radiation from high temperature explosion, Hopkinson designed an ingenious apparatus by which the flame radiation passed through a fluorite window in the explosion vessel, and was measured by means of a platinum strip bolometer. By this he determined that the loss of heat during the explosion period of 0·05 second amounted to nearly 5 per cent. of the whole heat of combustion. Hopkinson continued this investigation with his pupil David, and added greatly to our knowledge of the behaviour of flame as to loss by radiation at about 2000° C. His experiments also on the residual turbulence within the cylinder of internal combustion engines assisted materially in clearing up our ideas on the effect of the inlet velocity of the gaseous charge upon the economy and action of working engines.

" Colonel Hopkinson was most able and resourceful in all his experimental work, which threw much light on the phenomena of gas and petrol engines. He applied his great knowledge to the service of the Admiralty and the War Office while he was Secretary of the Engineering War Committee of the Royal Society, and later in his official position as Deputy Controller of the Technical Department in charge of aeroplane and aero-engine design and construction. His loss is keenly felt by his scientific colleagues and his associates of the Army and Navy."

In still another group of researches Hopkinson dealt with the dynamics of explosions from a different point of view. His paper on ' A Method of Measuring the Pressure produced in the Detonation of High Explosives, or by the Impact of Bullets," read before the Royal Society in November, 1913, and published in the 'Philosophical Transactions,' describes an investigation remarkable for its completeness no less than its originality. Taking the familiar method of the ballistic pendulum, which serves to measure the momentum of a blow, Hopkinson shows how to analyse this into its two factors, force and time, by means of a novel and ingenious variant. He used for the pendulum a steel rod divided by a transverse joint into a long and a short portion. The rod takes the blow longitudinally and transmits it as a wave of elastic compression which proceeds from the long piece to

the short one. At the extreme end of the short piece the wave of compression is reflected back along the rod as a wave of tension, and when the reflected wave reaches the joint the short piece flies off, carrying with it a fraction of the whole momentum which depends upon its length. By adjusting the length of the short piece it may be made to absorb the whole of the momentum of the blow, leaving the main portion of the rod at rest. This enables the length of the pressure wave to be determined, and from that the duration of the blow is readily inferred. Moreover, by using a very short length for the detachable piece the maximum pressure is also measured. The detachable piece is at first maintained in contact with the main portion of the rod by magnetic attraction, which keeps them together while the compression wave passes, but allows them to separate easily as soon as the stress at the joint changes into tension.

Applying this method to examine the blow given by a leaden bullet when fired so as to strike normally the end of the rod in the direction of the length, Hopkinson found that the results confirmed the view that the bullet behaved on impact like a fluid, producing at high speeds nearly the same pressure as could be produced by a fluid jet of corresponding velocity, density, and form. The duration of the blow was only slightly greater than that of such a jet. He discusses fully the causes of the small discrepancy, which is due in part to rigidity in the bullet and in part to the fact that the strain transmitted along the rod is not a simple wave of compression, uniform over the whole cross-section. But the discrepancy is so small as not to interfere with the value of the method as a means of measuring the force and duration of any blow. From the blows of bullets he passed to those caused by the detonation of gun-cotton near or close to one end of the rod, determined approximate limits of maximum pressure, and showed that at least 80 per cent. of the impulse of the blow had been delivered in one fifty-thousandth part of a second. The results detailed in the paper throw much light on the process of detonation and on the manner in which it produces its destructive effects.

Notably here, and scarcely less in some of his other papers, Hopkinson's scientific writing recalls that of his father. In saying this, one pays it a high tribute. There is the same absence of excrescence and verbiage and vagueness, the same avoidance of side issues, the same direct approach to the very core of the subject; there is the same impression of mastery and ease. Perhaps it is too terse; certainly it is so terse as to need very careful reading. But the careful reader is satisfied as well as convinced. What was said in this place* twenty years ago of the father's writings is no less true of the son's.

His writings, indeed, reflect the sincerity of the man. It was apparent to all with whom he came in contact; he quickly won their liking and respect. His nature was strong and self-reliant, singularly free from egotism or self-seeking. An obviously forceful personality, impressive in figure, in manner, in voice, he was conscious of his power, though not in the

* 'Roy. Soc. Proc.,' vol. 64, Obituary Notices, p. xxiv.

least vain of it. No doubt this contributed to another notable characteristic, that his good-humour was imperturbable. His temper kept sunny under the most trying conditions. Always a cheerful comrade, willing to take a full share of any duty, to respond to any demand, buoyant, frank, open, careless of self, he was a delightful associate in office or committee-room. One felt he lacked something, especially in his earlier years, of that comprehension of other men's idiosyncracies and weaknesses which is essential to perfect sympathy; but as time went on his experience, first as a teacher and later as an administrator, mellowed him. Among the men with whom he worked he was, it seemed, a universal favourite.

On the scientific side Hopkinson's strength lay, just as his father's did, in his combining a comprehensive grasp of principles with a just appreciation of practical requirements and possibilities. It was this combination that made him a successful head of the Engineering School at Cambridge, and determined the character of his researches; it was this again that made the value of his war work almost unique. The experiments of the Cambridge laboratory were of high interest in themselves and in their bearing on engineering practice. But to Hopkinson they were more; one may say they constituted an apprenticeship for the culminating work of his life, the work of the last four years. The war gave him an opportunity such as he did not have before. Into it he threw all his inventiveness, all his initiative, his untiring energy, his power of organisation, his unrivalled capacity for getting the best out of himself and out of others. No worker rejoiced more in his work nor accepted its call with more absolute self-renunciation. He was amazingly aloof from any consideration of private advantage or personal convenience. The strain was immense: the pressure of claims on his attention was continuous, but it never seemed to ruffle his serenity nor impair the soundness of his judgment. Many will mourn him as a trusted friend, but only those who knew something of what he did in the war can have a right idea of the magnitude of the nation's loss.

The President of the Royal Society, speaking as Master of Trinity in a Commemorative Sermon at the College, said of him: "Our Roll of Honour contains the name of no one who has rendered greater services to his country."

J. A. E.

Keith Lucas.

Hugo Müller

B. Hopkinson